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Thomas A. Hirst, Esq., Ph.D. University College, London,

were balloted for and duly elected Fellows of the Society.

On the Effect produced by the Angles of Position of Double Stars on the Results of Micrometrical Measures of them; with a Description of a Method by which such effect may be avoided or removed. By the Rev. W. R. Dawes.

Very soon after I commenced my measurements of double stars in the year 1830, I became aware of a tendency to obtain a different result in position when the line joining the centres of the stars was nearly *parallel* to the line joining the centres of the eyes, from that which was obtained when those lines were nearly *perpendicular* to each other; and a still more decided difference was found to prevail when those lines formed a very *oblique* angle. In discussing the points connected with these differences it will be convenient to call the position when *parallel* to the line joining the centres of the eyes *horizontal;* when *perpendicular* to that line, *vertical,* and when nearly half way between the two, *oblique.* Thus, a double star whose angle of position is near 90°, or 270°, would, when on or near the meridian, have what may be called a *horizontal* position;

if the position were about 0° or 180° it may be termed a *vertical* position; and if between 30° and 60° from the horizontal or vertical, the position would be *oblique*.

A tendency to a very considerable difference between horizontal and vertical measurements may exist without the observer being at all aware of it; and such a difference may to a great extent vitiate a large mass of observations without being suspected; except perhaps by comparison with the results obtained by other observers. But in such cases there will always be the great difficulty of determining which are the measures that have the strongest claim to be considered as true and standard ones.

In my own case I soon became aware of some difference in the results, which seemed to arise from the relative position of the stars; and the way in which I discovered it was this:—An *oblique* position seemed to me quite unsuitable for placing the wires of the micrometer parallel to the imaginary line joining the centres of the stars; and certainly, if it were wished, by aid of a ruler, to draw a line parallel to two points on paper, no one would, I should imagine, prefer to place the points in an oblique position for that purpose; but would, if possible, place them either in a vertical or horizontal position. When therefore I wished to measure such a star as ε *Boötis*, whose position-angle was about 50° from a horizontal and 40° from a vertical position, I performed the measurements when the star was sufficiently to the *east* of the meridian to bring the components nearly into a vertical line, and then obtained as nearly as might be an equal weight of observations when the star had arrived at a western azimuth sufficient to place its components nearly in a horizontal position; the large northern declination of the object permitting the two sets to be made within a few hours. Between the two results thus obtained I usually found a difference in the same direction, and pointing decidedly to the different positions of the star as its cause. I immediately set to work to devise means for ascertaining the amount of discrepancy arising from this cause; and also, if possible, for correcting the tendency to it. I considered that this would be preferable to proceeding with the measures, affected as many of them must be with an unknown amount of error; and then endeavouring by comparison of numerous results to calculate the amount of error in particular cases, and from this to devise a formula which might give a probable correction to all cases in which it was likely to be needed.

A very simple expedient soon occurred to me, which may be conveniently expressed in the form of directions to any one wishing to adopt it. Procure a moderately stout piece of cardboard. Out of this cut a triangular piece of any convenient size,—two of the sides being as accurately as possible at right angles to each other. The two acute angles may either be of 45° each, or more usefully of 30° and 60°. In this card make

some small apertures; the larger ones with a very fine and sharp punch, and the smaller with a hot needle. It is well to have several pairs of holes, some of equal size and moderate distance to begin with, and others, for more advanced practice, of unequal size and smaller distance. Supposing the acute angles of the triangle to be 30° and 60°, it will be convenient to designate the side opposite the angle of 30° by *B* (the base), the other side by *P* (the perpendicular), and the hypothenuse by *H*. In this form of triangle there is no necessity for so many sets of holes to vary sufficiently the angles of position; for a great variation is produced in most of them by merely reversing the side of the card presented to the observer. The lines joining the centres of the apertures may be drawn at any angle with respect to *B*; but I should strongly recommend that one set of each kind (equal and unequal size, and at different distances) should always be made as exactly as possible parallel to *B* or *P*; and also one of each parallel to *H*. Of course, to prevent the confusion which might arise from too many sets of holes in one card, as many cards may be prepared as the observer may think convenient.

A card thus prepared and set up on the frame of an ordinary window may be viewed with a small telescope in a room of moderate size. I employed an excellent 2-foot object-glass by Dollond, aperture 1·6 inch, mounted in a pretty stout tube, with another sliding into it having a screw which fitted my parallel-wire micrometer, also by Dollond. This was firmly fixed in a stout clip upon a floor tripod; the clamp of the clip holding the tube so tightly as to prevent any accidental rotation on its axis.

The use of the apparatus is now so obvious as to need little further explanation. I first began with an equal and moderately wide pair in a vertical position, taking a few measures of it with the card standing on the side *B*; and then an equal number of the same object placed horizontally by turning the card on to the side *P*. The means of several sets of careful measures in each position gave accordant results; the difference between the vertical and horizontal measures, even when not large, always lay in the same direction; and thus I was quickly led to perceive its cause and its amount on objects variously constituted. Having thus determined the existence and the amount of the errors, I endeavoured to remove the cause of them by taking measures as accurately as possible in a vertical position, and then altering the reading of the micrometer 90° from the mean, the card being also turned from *B* to *P*. This at once revealed the error of judgment, which I have always considered to belong to the horizontal and all other positions rather than to the vertical, which were, at all events pretty uniformly the most accordant among themselves.

Each observer may, of course, pursue some such plan of observation on any number of *oblique* positions; and thus en-

deavour to divest his judgment of all discrepancies attaching to them. But it presently occurred to me that such a process would be unnecessarily long and laborious; and that at the same time an equally satisfactory result might be obtained in a simpler way, namely, by the use of a small prism attached to the eye-piece between it and the eye. This I accordingly had done by Dollond; and found it work so satisfactorily that I strongly recommended its use in the Introduction to my first series of Double-star Measures, which was read before the R.A.S. in 1834, and honoured with a place in vol. viii. of the *Memoirs*. This simple contrivance, so easily managed, enables the observer to place any double-star in any desired position with respect to a vertical or horizontal line; and in my own observations I have frequently resorted to it even with stars whose position is not very oblique (as *e.g. Castor* and *γ Leonis*), in order to assure myself that I still retain the uniformity of judgment which I acquired by practising with the artificial stars in the cardboard. I have however confined myself entirely to the vertical and horizontal positions.

It is a remarkable circumstance that that excellent and experienced observer, M. Otto Struve, after having accumulated an immense mass of double-star measures, became aware that they were liable to a considerable error, in many instances at least, from obliquity of position; and in order to ascertain its amount and the law by which it was governed, he instituted a careful series of measures of artificial double stars set up at a considerable distance from the Poulkova Observatory, and observed with the large equatoreal. This is, of course, an incomparably more laborious plan, and can be pursued only under extremely favourable circumstances. There must also exist other rare and peculiar combinations to render it at all possible to affix suitable objects at a sufficiently great distance and in a suitable light, &c. The results however, procured with great care and labour, proved very important, and showed the necessity in many instances of applying a considerable correction to the observed angle, and a formula is given for its computation. In his paper on this subject he refers to my recommendation of the use of a *prism;* but raises the objection that a prism might introduce errors of a different kind, and in every case might injure the quality of the telescopic images. "This," he adds, "is the reason why astronomers in general have not adopted the proposal of Mr. Dawes, which under certain circumstances is worthy of being pursued, because it might furnish interesting corrections (or modifications, *contrôles*) to the results found by another method."

With reference to this objection to the use of the prism, which would be formidable if well founded, I feel bound to say that, having during the thirty-five years which have elapsed since my first proposal and use of it employed prisms made by Dollond, Merz, Simms, and the late Andrew Ross, I have found

that no one of them produced any perceptible deterioration of the image. I cannot therefore but conclude that this fear has no foundation sufficient to deter any observer from the use of this instrument; and nothing can be more easy of detection than a fault of this nature. Moreover, the light lost is so small that extremely delicate objects may be observed with it. M. Otto Struve further states that he is not aware that I have published any results affording a comparison between the results obtained with the prism and without it. It is quite true that I have not published any such; the reason of which was simply that I supposed the defect was probably restricted to my own observations, never having at that time (in 1830) heard of any observer who had detected a similar source of error. Neither was this point elucidated by any allusion to it in a correspondence which ensued between Sir John Herschel and myself. I therefore concluded that it was probably peculiar to my own observations; and having succeeded in correcting the tendency to it by the method explained in this paper, I did not suppose that the subject would possess much interest for other observers. Lately, however, my attention has been again attracted to the subject by a correspondence with one of our best double-star observers; and I have therefore thought it desirable no longer to delay the present explanation of my own successful method of overcoming the difficulty.

Hopefield Observatory, Haddenham, Bucks,
6 *June*, 1866.

Equatoreal Observations of R.A. of Mars and neighbouring Stars for the Determination of the Sun's Parallax made with the Equatoreal of the Observatory at Madras in the year 1862, *and Meridional Observations of N.P.D. of Mars and neighbouring Stars made with the Meridian Circle.* By N. R. Pogson, Esq., Government Astronomer. (Abstract.)

At the request of the Astronomer Royal, observations of the Planet *Mars* were made during the opposition of 1862, at rising and setting, with a view of illustrating the method suggested by him in the *Monthly Notices of the Royal Astronomical Society*, vol. xvii., p. 219, and again in vol. xviii., p. 277, for the correction of the Constant of Solar Parallax, rather than with any hope of being able to realise a trustworthy result, owing to the very imperfect instrumental means available for such observations. A similar proposal had been made to Major

Tennant, Government Astronomer at Madras, in regard to the opposition in the year 1860, but had also been prudently declined by him on the very grounds now urged and foreseen by the present Astronomer before commencing the series under consideration.

The instability of the Madras Equatoreal; its damaged and shattered condition, owing to the effect of repeated storms, from the fury of which the worthless folding roof over it was literally no protection at all; its defective and in part useless illumination; all combined to render every possible exertion and precaution alike nugatory, and to reduce the attempt to a mere justification of Major Tennant's judgment on the former occasion.

Observations (114 in number) were, however, made, with 20 Comparison Stars, between September 22 and October 28, 1862; and though, for the reasons above stated, the result could not be expected to possess any actual value, it nevertheless proves clearly that the long-adopted value of the parallax requires to be increased by a considerable fraction of itself; and that the method, if effectually carried out by the aid of better instrumental means, is in itself good, and well worthy of the utmost care and preparation, when the time shall again arrive for repeating the measurements of *Mars* under more favourable circumstances than those existing at the Madras Observatory in the year 1862.

The final value of x (Correction to Parallax), viz. $+0''{\cdot}579$, is evidently too large; giving $\pi = 9''{\cdot}156$. The Comparisons of *Mars* with four of the twenty stars did not enter into the determination at all, in consequence of these being only taken on one side of the meridian. All other observations are, however, included, and possibly a judicious weeding out of all dubious comparisons might give a better result. It may also have been wrong to correct only the mean values of $\alpha' - \alpha$ for refraction and parallax; each individual comparison, or at least those of not more than ten minutes apart, being better taken separately. It is not, however, imagined that much improvement would have arisen from so vastly extended a system of corrections; and the failure of the attempt is probably attributable to the too low magnifying power employed (only 63) rather than to any other cause; and much as the unsatisfactory result is regretted, the author exerted his most earnest endeavours to do the best he could with the very imperfect and unsuitable instrumental means available. He remarks that any suggestions as to a better mode of treatment of the observations would be thankfully received and acted on at the earliest opportunity.

Occultations of Stars by the Moon, and Phenomena of Jupiter's Satellites, observed at the Royal Observatory, Greenwich, from April 1864 to April 1866.

Occultations of Stars by the Moon.

Day of Observation.	Phenomenon.	Moon's Limb.	Mean Solar Time.	Observer.
1864.			h m s	
June 26	62 Piscium, disapp.	Bright	13 22 29·6	R.
	(*a*) δ Piscium, disapp.	Bright	13 40 7·1	R.
	(*b*) 62 Piscium, reapp.	Dark	14 11 32·6	R.
	(*b*) δ Piscium, reapp.	Dark	14 40 27·5	R.
Dec. 5	κ Aquarii, disapp.	Dark	8 34 54·8	J.C.
	(*c*) κ Aquarii, reapp.	Bright	9 28 17·9	J.C.
1865.				
Mar. 3	δ^2 Tauri, disapp.	Dark	10 15 20·9	J.C.
July 3	α^2 Libræ, disapp.	Dark	9 58 18·0	J.C.
	(*d*) α^2 Libræ, reapp.	Bright	10 50 33·2	J.C.
8	(*e*) ξ^1 Sagittarii, disapp.	Bright	9 21 8·0	J.C.
Nov. 4	δ^3 Tauri, reapp.	Dark	10 36 44·9	J.C.
Dec. 30	115 Tauri, disapp.	Dark	7 34 58·9	L.
1866.				
Feb. 23	130 Tauri, reapp.	Bright	6 23 46·1	C.
27	*h* Leonis, reapp.	Bright	9 0 43·5	C.

(*a*) The star appeared to hang about $0^s{\cdot}7$ before disappearance. (*b*) Instantaneous. (*c*) A little separated from the limb when first seen. (*d*) The star appeared to hang on the limb about $\frac{1}{2}^s$ or $\frac{3}{4}^s$ after reappearance. (*e*) A very unsatisfactory observation.

Phenomena of Jupiter's Satellites.

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time.	Observer.
1864.			h m s	
April 14	I	(*f*) Eclipse, disappearance	13 33 44·2	C.
May 7	I	(*g*) Eclipse, disappearance	13 44 21·3	J.C.
16	I	(*g*) Occult. disapp. bisection	9 58 35·4	J.C.
22	III	Eclipse, reappearance	9 34 39·0	E.
23	I	Occult. disapp. first contact	11 41 52·3	C.
	I	Occult. disapp. last contact	11 46 36·5	C.
	I	Eclipse, reappearance	14 9 7·2	C.
29	III	Eclipse, reappearance	13 33 19·1	C.
June 13	II	Transit, ingress, first cont.	9 2 8·0	C.
	II	,, bisection	9 4 37·6	C.
	II	,, last cont.	9 6 37·1	C.
	II	Transit, egress, bisection	11 20 40·3	C.
	II	,, last cont.	11 22 40·0	C.

Day of Observation. 1865	Satellite.	Phenomenon.	Mean Solar Time. h m s	Observer.
May 22	II	(*h*) Occult. reapp. bisection	13 34 30·7	J. C.
June 11	I	(*h*) Eclipse, disappearance	13 12 17·8	J. C.
19	I	(*h*) Transit, ingress, bisection	12 6 54·8	J. C.
20	III	Eclipse, reapp. bisection	12 41 41·9	E.
28	I	(*h*) Transit, egress, last cont.	10 32 39·0	C.
July 25	II	Eclipse, reappearance	10 41 32·9	C.
29	I	Eclipse, reappearance	10 20 28·5	T. C.
Aug. 2	III	(*i*) Eclipse, disappearance	10 4 9·5	L.
Sept. 2	II	Occult. disapp. first cont.	7 51 50·5	E.
	II	,, bisection	7 53 20·3	E.
6	I	(*i*) Eclipse, reappearance	8 50 51·4	C.
7	III	Eclipse, reappearance	8 43 54·1	E.
14	III	(*k*) Occult. reapp. bisection	7 34 26·7	J. C.

(*f*) Unsatisfactory; cloudy. (*g*) Very uncertain; the image of the planet bad. (*h*) The observation very unsatisfactory; the planet very badly defined. (*i*) Somewhat uncertain, the satellite being very faint near *Jupiter*, which was low. (*k*) Unsatisfactory from haze near the horizon.

The initials D., E., C., L., J. C., R., and T. C., are those of Mr. Dunkin, Mr. Ellis, Mr. Criswick, Mr. Lynn, Mr. Carpenter, Mr. Roberts, and Mr. Chappell.

On the Change that would take place in the Elements of the Earth's Orbit by a sudden accession to the Sun's Mass.

By John James Waterston, Esq.

That the appearance seen in the Sun on the 1st of September, 1859, by Mr. Carrington and by Mr. Hodgson, indicated an accession to the Sun's mass is no doubt very generally admitted, but that the amount of that accession can have sensibly diminished the length of the year is an idea that may perhaps incur ridicule, not from its impossibility, but because the Greenwich observations must ere this have detected so remarkable a circumstance. There seems also to be an impression that the fall of such a planet as the Earth into the Sun "would, from the conversion of its previous mechanical energy, give out an awful blaze of light, such a blaze as . . . would scorch up in a moment all the inferior planets, and probably the Earth also, with everything upon it" (*Monthly Notices*, vol. xx. p. 89).

When this is put to test of figures, as at p. 197 of the same volume, such a catastrophe is clearly out of the question. Even if the blaze were persistent, the general rise of the temperature could not exceed 10° or 15°, but it could not be persistent, inasmuch as the extra power of radiation can only act for the short time taken to traverse the Sun's atmosphere before plunging under its surface, which observation shows to be not only in-

candescent but highly mobile as a fluid; the blaze would then terminate, and the general temperature of that part of the Sun be very sensibly increased, yet not so as to increase the radiating power in a perceptible degree.

Take the extreme case of the whole potential radiating power of the Sun being thereby raised 1000°, even this is only $\frac{1}{12\,000}$th part of the potential temperature that sends heat to us sufficient to maintain a general average temperature over the surface of the Earth of about 500° above the absolute zero. Now this proportion of 500° is only $\frac{1}{24}$th of a degree, and this is the extreme maximum effect that can be reasonably expected from such a planet fall.

The phenomenon observed on the 1st September is thus quite consistent with what might justly be expected from an accession to the Sun's mass approaching in magnitude to our planet. Mr. Carrington describes the intensity of the blaze to be fully as great as if there had been a hole in the screen attached to the object-glass of the telescope; fully as great as the direct unscreened sunlight. Mr. Hodgson describes it as being most dazzling to the protected eye. Such descriptions warrant an estimate of intensity several hundred times that of the normal surface; enough if persistent to produce a considerable change of climate; but as persistence is physically impossible, nothing of the kind need be looked for. The only indication of the mass thus added to the controlling centre of the system which we can become cognizant of is the possible decrement in the length of the year.

The following is the process of computation I have employed to ascertain this. The resulting formula, it will be remarked, is extremely simple in consequence of the excentricity of the Earth's orbit being small.

If the mass equalled that of our globe the decrement in the length of the year is 130^s. This would cause a difference of $5''{\cdot}3$ in the longitude of the Sun at the end of the first year (September 1, 1860), of $10''{\cdot}6$ on September 1, 1861, and on September 1, 1866, the Sun would be $37''$ in advance of the position given in the *Nautical Almanac*. If the mass was less than this the difference would also be less in the same proportion.

In fig. 1 the major axis of orbit is P S A; the triangle formed by the Sun, Earth, and aphelion focus is S E F; the radius of curvature is R E, and the normal E N.

An addition of $\frac{1}{m}$th to the Sun's mass causes an immediate decrement of $\frac{1}{m}$th in the length of R E and a change in the length of the normal, but no change in the direction of R E or F E.

Suppose a line to be drawn from S, passing through N′,

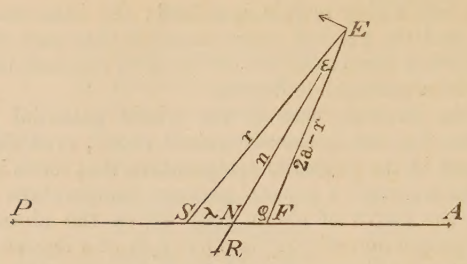


Fig. 1.

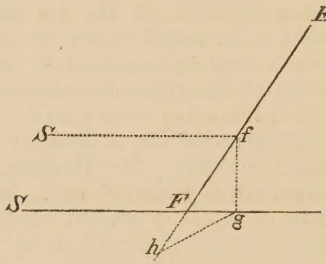


Fig. 2.

the new position of N, and produced to meet F E in f (fig. 2). Draw $f g \perp$ S F and make F h = F g.

The incremental change in the direction of the major axis is $\frac{fg}{SF} = \delta\lambda$. The decrement of major axis $2\delta a$ is Ff = $-2\delta p - 2\delta e$ (p being the perihelion distance P S and and $2e$ = S F). The increment of $2e$ is Fg = Fh, and the decrement of $2p$ is fh.

Let ϕ represent $\angle$ E F S, $\lambda = \angle$ E S F, and $\varepsilon = \angle$ E, $fg = 2e\delta\lambda$, Fg = $\cot\varphi\ 2e\delta\lambda = 2\delta e$, F$f$ = $\operatorname{cosec}\varphi\ 2e\delta\lambda = 2\delta a$.

With δa represented by x, we have

$$\delta\lambda = \frac{x \sin\varphi}{e} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\delta e = x\cos\varphi \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\delta p = x - x\cos\varphi \quad . \quad . \quad . \quad . \quad (3)$$

The radius of curvature ϱ is equal to the cube of the normal n $\left(= \frac{r \sin\lambda}{\sin(\lambda + \frac{\varepsilon}{2})}\right)$ divided by the square of the semiparameter s $\left(= \frac{b^2}{a}\right)$ hence

$$\frac{\delta \varrho}{\varrho} = \frac{1}{m} = \delta \left(\frac{n^3}{s^2}\right) \cdot \frac{s^2}{n^3} = \frac{3\delta n}{n} - \frac{2\delta s}{s} \quad . \quad . \quad (4)$$

The first term

$$\frac{3\delta n}{n} = 3\delta\lambda \cdot \frac{\sin \frac{1}{2}\varepsilon}{\sin\lambda \sin(\lambda + \frac{1}{2}\varepsilon)} = \frac{3\delta\lambda}{\sin^2\lambda \cot\frac{1}{2}\varepsilon + \sin\lambda\cos\lambda} \quad (5)$$

In the triangle S E N, the angle ε being small, we have

$$\frac{n \sin\frac{1}{2}\varepsilon}{\sin\lambda} = e \,(\textit{nearly}) \text{ and } \sin\frac{1}{2}\varepsilon = \frac{e}{r}\sin\left(\lambda + \frac{1}{2}\varepsilon\right)$$

hence

$$\cot\frac{1}{2}\varepsilon = \frac{r}{e\sin\lambda} - \cot\lambda$$

substituting the value of $\cot\frac{1}{2}\varepsilon$ in (5) we have

$$\frac{3\delta n}{n} = \frac{3e\delta\lambda}{r\sin\lambda} = \frac{3x}{2a-r} \cdot \quad . \quad . \quad . \quad . \quad (6)$$

The second term

$$-\frac{2\delta s}{s} = -\frac{4\delta b}{b} + \frac{2x}{a}$$

and since $b = \sqrt{a^2 - e^2}$ we have

$$-\frac{4\delta b}{b} = -4 \cdot \frac{ax - e\delta e}{a^2 - e^2} = -\frac{4x}{a} \,(\textit{nearly})$$

hence

$$-\frac{2\delta s}{s} = -\frac{2x}{a} \quad . \quad . \quad . \quad . \quad (7)$$

and (5), (6), (7),

$$\frac{1}{m} = -x\left\{\frac{3}{2a-r} - \frac{2}{a}\right\} \text{ or } x = \frac{2a-r}{2r-a} \cdot \frac{a}{m} \cdot \quad . \quad (8)$$

Computing this for September 1, 1859, with $m = 354936$ we obtain $x = 261$ miles, which, compared with a and the length of the year, is found to represent a decrement of this length equal to 130 seconds.

The change in the values of λ, e, and p, may be ascertained from (1), (2), (3), $\delta\lambda = -29''$, $\delta e = +137$, $\delta p = -398$.

Supposing many such planet-falls to have happened in the history of the Sun, and to take place at every different part of the orbit, the motion of the apses would be cumulative in the direction of the signs.

If the accession took place in July $x = -255$ and $\delta e = +255$.

If it took place in December $x = -281$ and $\delta e = -281$. The change of e is (+) at the upper half of the orbit and (−) at the lower.

Inverness, April 25, 1866.

Observations of the extraordinary Variable lately discovered near ε Coronæ. By E. J. Stone, M.A.

This star has been observed with the Greenwich Transit-Circle on every clear night since May 17. Its mean place for 1866, Jan. 1, is as follows:—

R.A.	N.P.D.
$15^h\ 53^m\ 53^s{\cdot}8$	$63°\ 41'\ 52''{\cdot}9$

In Argelander's *Bonner Sternverzeichniss*, Zone + 26°, No. 2765, will be found a star of 9·5 magnitude, whose mean place for 1855, Jan. 1, was

R.A.	N.P.D.
$15^h\ 53^m\ 26^s{\cdot}9$	$63°\ 39'\ 54''$

The mean place of this star for 1866, Jan. 1, without any allowance for proper motion, would be

R.A.	N.P.D.
$15^h\ 53^m\ 54^s{\cdot}5$	$63°\ 41'\ 49''$

There is certainly no star of the 9th magnitude near the variable. It is clear, therefore, that the variable is really the star No. 2765 of Argelander's Zone + 26°; and that at the time of observation in Argelander's sweep it was below the 9th magnitude.

This star was seen, on May 12, by Mr. Birmingham, of Tuam, as a star of the 2nd magnitude.

It was observed by Dr. Schmidt, at Athens, on May 13. Later on the same evening it was observed also by M. Courbebaisse, at Rochefort. Mr. Barker saw it at London, Canada West, on May 14, and on May 15 it was independently discovered by Mr. Baxendell, of Manchester. Notice of the discovery was now circulated, and the attention of astronomers directed to the star. It is very remarkable that M. Courbebaisse is of opinion that the star could not have been conspicuous to the naked eye on May 11, and he is confident that such was not the case on May 9. Mr. Baxendell, also, is confident that the star was not sufficiently bright to attract attention with the naked eye on May 7. It is, however, extremely difficult to prove a negative, and we may even yet hope to receive some earlier observations of this star. The number of independent discoveries of this star is not a little remarkable, and proves how widely spread is the love of our science, and with what minute care and knowledge the heavens are nightly scanned.

The following table of estimated magnitudes contains all the observations which have come to my hands. It lays no claim to completeness, and is published merely with the hope of inducing those gentlemen who have made estimations of brightness to publish their results.

Estimations of Magnitude.

	Barker.	Baxendell.	Birmingham.	Carpenter.	Courbebaisse.	Dawes.	Schmidt.	Stone.	Observers with Greenwich Transit-Circle.
May 12	...	...	2	...	...	...	...	...	...
13	...	...	...	...	2–3	...	3–2	...	...
14	3	...	...	...	...	...	...	...	...
15	...	3·6	...	...	...	...	3–4	...	...
16	...	4·2	...	...	...	...	...	...	...
17	...	4·9	...	4·5	...	...	...	...	...
18	...	5·3	...	...	...	4·7	...	4·8	...
19	...	5·7	...	...	...	5·2	...	5·0	...
20	...	6·2	...	...	...	...	...	...	...
21	...	...	...	...	...	6·3	...	...	...
22	...	...	...	7·2	...	...	...	7·2	7·5
23	...	...	...	...	...	7·2	...	7·8	7·5
24	...	...	...	...	...	...	...	8·4	8·5
25	...	...	...	...	...	...	...	...	...

	Barker.	Baxendell.	Birmingham.	Carpenter.	Courbebaisse.	Dawes.	Schmidt.	Stone.	Observers with Greenwich Transit-Circle.
May 26	...	...	...	...	...	...	...	...	...
27	...	...	...	...	...	...	...	...	8·5
28	...	...	...	...	...	...	...	8·2	8·3
29	...	...	...	...	...	...	...	...	8·5
30	..	...	...	...	...	...	...	...	...
31	...	...	...	...	...	...	...	...	...
June 1	...	...	...	...	...	..	...	...	...
2	...	...	...	...	...	...	...	...	...
3	...	..	...	...	...	...	...	...	...
4	...	...	...	...	...	...	...	...	...
5	...	...	...	...	...	...	...	...	...
6	...	...	...	...	..	...	...	...	...
7	...	...	...	...	...	...	...	8·9	...

The Greenwich observations are based on Argelander's magnitudes.
On May 24 the sky was not in a good state for observing magnitudes.

It appears that the diminution of brightness was for some time at the rate of about half a magnitude a-day: towards the end of May the decrease was far less rapid. I have never noticed any traces of nebulosity in the star; it has always appeared to me to come up quite sharply to focus. This is also the opinion of all the observers at Greenwich.

On May 19 the Astronomer Royal's spectrum apparatus, by which the star-spectra are referred directly to the fixed lines of the Sun's spectrum, was mounted on the Great Equatoreal. On turning the instrument upon the star, it was at once seen that the ordinary star-spectrum was crossed by four bright lines; three of these lines were sufficiently bright for position measurement: the position of the fourth, as laid down in the diagram, was only estimated. The magnitude of the star at the time of observation was about the 5th.

The following excerpts from the observing-books are published with the sanction of the Astronomer Royal, who saw the spectrum himself on May 19:—

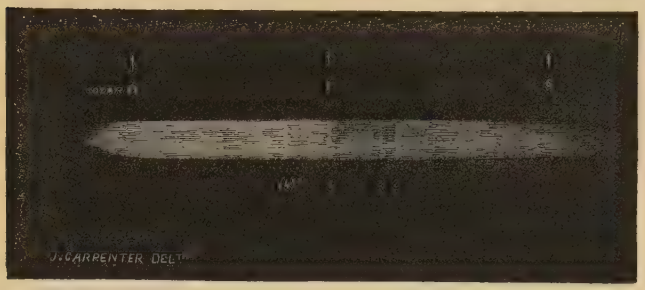
1866, May 19, 11^h.

Observers, E J. Stone and J. Carpenter.

Observations of Spectrum of New Variable in *Corona*.

The spectrum appeared to consist of two parts: one an ordinary star-spectrum, in which were traces of absorption-

lines; the other, a discontinuous spectrum, consisting of four bright lines, of which the positions of only three were measured. The position of the fourth was fixed by estimation.



The visible spectrum extended from about D to half way between G and H of the solar spectrum. The contrast between the brightness of the lines and the adjacent parts of the continuous spectrum was very remarkable.

When the instrument is in adjustment, the reading for the solar time F is $89^{r}\cdot999$, and 20^{r} of the micrometer carries the index from F to G.

The following are the micrometer-readings for the three lines measured:—

For 1	90·486	From 1 − 2 = 5·760
2	84·726	From 1 − 3 = 7·072
3	83·414	

May 20, 12^{h}.

Observers, E. J. Stone and J. Carpenter.

The following are the micrometer-readings:—

For line 1	90·407	From 1 − 2 5·756
2	84·651	

The readings have each been diminished one revolution.

May 22, 11^{h}.

Observers, E. J. Stone and J. Carpenter.

Observation of absorption line in the spectrum of *α Coronæ* coincident with the solar line F, for verification of zero. The reading was

$90^{r}\cdot003$.

Observation of line 1 of the spectrum of Variable: the reading was

$89^{r}\cdot658$;

observation difficult.

May 23, 11^{h}.

Observer, E. J. Stone.

The lines 1, 2, 3, and (4), were all seen, but the positions of only 1 and 2 were measured.

Reading of Micrometer for 1 =	$90^{r}\cdot218$
2	$84\cdot325$
For 1 − 2	$5\cdot893$

May 24, 11^{h}.

Observer, E. J. Stone.

The reading of micrometer for line 1 was

$90^{r}\cdot346$.

Observation very difficult; strong moonlight, and at times haze.

May 28.

Observer, E. J. Stone.

The star was now of about the 8·2 magnitude. The moonlight was overpowering, but I have no doubt about the presence of the bright line 1. I tried several times to coax the micrometer up to the part of the field where I thought the line was. The micrometer was at each trial found reading about $90^{r}\pm$, or near the true place of the line.

June 7.

Observer, E. J. Stone.

The line near F was certainly still visible, but desperately faint.

The mean of all the readings for line 1 is

$90^{r}\cdot223$.

The reading for the solar line F is

$89^{r}\cdot999$.

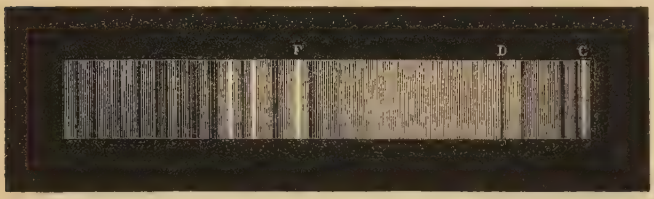
The difference is too small a quantity to be answered for in observations of such difficulty. It is clear that the line 1

is either identical, or almost identical, in position with the solar line F. The diagram has been laid down assuming the bright line to be coincident with F. The unchanged character of the spectrum, when the star had decreased much below the 8th magnitude, appears to me a point of much importance.

It was the opinion of Mr. Carpenter and myself that from the 19th of May the brightness of the gaseous and ordinary spectra decreased in very nearly, if not quite, the same proportion.

1866, *June* 8.

Diagram of the Spectrum of Absorption and the Spectrum of Bright Lines forming the Compound Spectrum of the temporarily Bright Star near ε Coronæ Borealis.



This diagram, taken from the *Proceedings of the Royal Society*, is given, by the permission of that Society, in illustration of Mr. Huggins's letter to the Editor at page 275 of the last Number of the *Monthly Notices*.

Extract of a Letter from Mr. Graham.

The star in *Corona Borealis* which has attracted so much attention is a Variable star of wide range of magnitude, but is not new. It is marked in Argelander's comprehensive approximate Catalogue $9\frac{1}{2}$ magnitude, and the place, referred to the mean equinox of 1855·0, is

R.A.	h m s 15 53 26·9	Decl.	° ′ 26 20·1 N.
		N.P.D.	63 39·9

These for 1866·0 become

R.A.	h m s 15 53 54·3	Decl.	° ′ 26 18·8 N.
		N.P.D.	63 41·9

The place obtained by the meridian instruments at this Observatory, on the 17th instant., referred to the same epoch, is

R.A.	$15^h\ 53^m\ 53^s\cdot85$	Decl.	$26^\circ\ 18'\ 9''\cdot8$ N.
		N.P.D.	$63\ 41\ 50\cdot2$

Remarkably enough, in Wollaston's Catalogue, the epoch of which is 1790, there is an object recorded thus:—

R.A.	$14^h\ 51^m \pm$	N.P.D.	$63^\circ\ 29' \pm$

with the following note:—

"Double (Hers. v. 75) v. v. uneq. . . dist. 41″ 12‴ . . . pos. 16° *s. f.* It is really quadruple, for the small star is double, and there is a still smaller at about 30° *s. p.* the small ones."

This place reduced to 1866·0 becomes

R.A.	$15^h\ 54^m$	N.P.D.	$63^\circ\ 42'$

which also accords with the star in question.

Another circumstance worthy of record is that a nebula is marked on Cary's large celestial globe as nearly as possible in the very spot occupied by this star, and which is not in Herschel's Catalogue.

Cambridge Observatory,
25th May, 1866.

The New Variable Star near ε Coronæ.

By G. F. Chambers, F.R.A.S.

I should hardly have ventured upon troubling the Society with this communication had it not been for some remarks on the new star, made by Messrs. Hind and Talmage in letters addressed to a daily London newspaper; it seems to me that in dealing with scientific matters every observation *bonâ fide* made, however seemingly discordant with previous ones, should be placed on record. The following are my notes:—

May 21. Turned telescope (aperture 4 inches, powers 40 and 60) on the assigned place of the new star, and found the same immediately. Made a diagram of the neighbourhood. (x) is the stranger. It is about equal in brightness to (y), a star south of and in the field of ε *Coronæ*, but is brighter than (z), star south preceding π *Serpentis*. Hind speaks of the new star as free from colour, but to my eye it appeared from the very first moment of my seeing it to have a distinct pale orange tinge. It also struck me as particular sharp and well defined.

Talmage however says the contrary; that, on the 18th at any rate, it was surrounded by a hazy nebulosity 30″ in extent. My own idea would be that if ever I had seen a star less likely under superior aperture to reveal a trace of nebula, this is that star. I hope to find light thrown on these discrepancies in some of the many papers which I suppose will be read at the next Meeting of the Society.

May 22. New star less bright and orange tinge less strong, but I consider perceptible, though the Rev. T. W. Webb, who was with me, did not notice it. He quite concurred however about the sharpness of the star's image.

June 2. The star has considerably diminished in size, and the orange tinge is no longer striking; I do not however consider the star destitute of a yellowish cast.

The two stars ε *Coronæ* and π *Serpentis* are I see rated in the Catalogues each as of $4\frac{1}{2}$ mag., but it occurs to me as just worth mentioning that the latter is considerably less bright than the former.

Attention has, I believe, long ago been drawn to the fact that no inconsiderable number of the important known variables present at their maximum red, orange, or yellow lines. It seems to me that this circumstance is worthy of more notice than has hitherto been given to it, and I cannot but fancy that some interesting but recondite physical law lies concealed here under the surface.

The Observatory, Sydenham, Kent,
June 4, 1866.

On a Supposed Observation of the New Variable near ε Coronæ.

(*Extracts from a Letter of Sir J. F. W. Herschel, Bart., to one of the Secretaries.*)

"At the suggestion of the Astronomer Royal, I send you herewith, for communication to the Royal Astronomical Society, a sheet pricked off and accurately copied from the original, containing my naked-eye estimations of the magnitudes of all the stars which I was able to discover on the night of the 9th of June, 1842, within the areas of five triangles, marked out by the stars η, ζ, β *Herculis*, γ, β *Serpentis*, α *Coronæ*, and β, δ *Boötis*.

"Within one of them occurs a star marked as of magnitude 6′, that is to say, according to the system of notation adopted, $6\frac{1}{3}$ or 6·7 magnitude, as distinguished from 7·6 (a magnitude clearly distinguishable by the naked eye in the absence of the Moon) and whose place, as laid down by allineations with the neighbouring stars δ, ε *Coronæ* and π *Serpentis*, agrees so nearly with that assigned to the star whose recent outbreak has

excited so much attention, that I cannot help believing it to be the same."

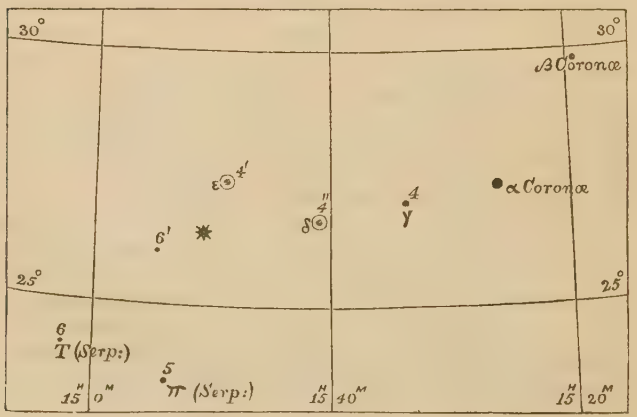
"The sheet I annex is copied from one out of a great series of similar ones, forming part of a series of naked-eye observations for the estimation of magnitudes and detection of variable stars, carried out in both hemispheres; in which the heavens were divided out into 738 triangles, with a design of mapping into them, *seriatim*, every visible star. The working-sheets were, in each case, pricked down from Bode's Atlas, and all the leading stars (including all of the 5th and a great many of the 6th magnitudes) laid down in that Atlas pricked in."

"The meridians and declination circle, drawn on a sheet, correspond, of course, to those in Bode's charts and to the epoch of January 1, 1801, adopted in that work. The stars not in Bode's charts (which are very numerous) were all corrected with as much care and endeavour at precision as could conveniently be bestowed on them in a work of the kind, but of course their positions are open to a good deal of error. In the case of the star in question however there can be no mistake as to the star, there being no other of equal brightness within a degree and a half of its place. If it be not the new star it is a Variable star which merits attention for its own sake.

"In an old celestial globe by Bardin, in my possession, containing 'the positions of 6000 stars, clusters, nebulæ, &c., &c., laid down from the latest observations of Maskelyne, Dr. Herschel, the Rev. F. Wollaston, &c., computed and reduced to the year 1800,' I find a star marked as 9th magnitude, laid down exactly in the place of mine of 6th mag."

Collingwood, May 28, 1866.

Note.— A portion only of the sheet has been engraved. The position of the variable is inserted and marked thus *.



On the Advantages gained by substituting a Reflecting Prism for the Diagonal Mirror in a Silvered-Glass Speculum. By John Browning, F.R.A.S., F.M.S.

In the paper which I read at the Meeting of January 9th, on a new method of mounting silvered glass specula in reflecting telescopes, I recommended the use of a prism, in preference to a plane silvered glass diagonal mirror, for reflecting the image into the eye-piece. Since then many objections have been made to the employment of a prism for this purpose.

It has been said, firstly, that dew is deposited on a glass prism much more readily than on a metallic surface. Secondly, that, in consequence of three surfaces, instead of only one, being brought into action, and the image having to pass through the glass, thus being influenced by all its defects, the difficulty of making the prism perform as well as the plane mirror is almost insurmountable. Thirdly, that the prism reflects no more light than the silvered mirror. This last statement has been made on Steinheil's authority, whether justly or not I cannot say.

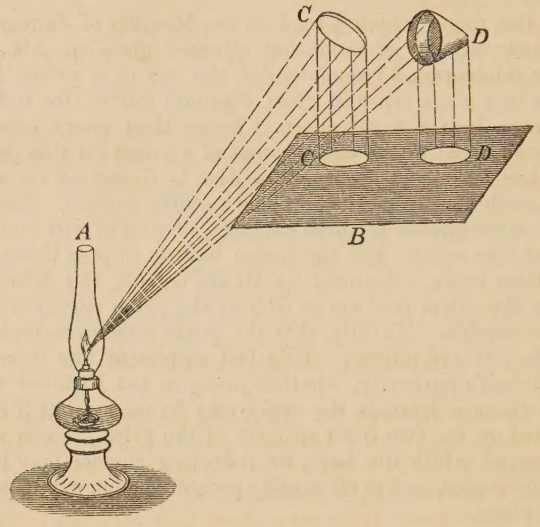
To these objections the reply may be made, that if dew be deposited on the two front surfaces of the prism, it can readily be removed while the back or reflecting surface may be hermetically sealed, and so effectually protected from any deposition taking place.

Should moisture appear, however, on the surface of a silvered glass reflector, as is well known, it will greatly injure, if it does not entirely destroy, the film.

It is evident that any deterioration of the surface of this mirror will be much more prejudicial than the same amount of injury to the surface of the large speculum, from the cone of rays being concentrated on the small surface; and its exposed position near the mouth of the tube renders it far more liable to be injured in this manner than the large speculum. I have not made any experiments to determine the exact amount of light reflected relatively by the prism and the silvered glass mirror, because the amount would vary with every silvered mirror tested; but I think, simply viewing a white or greyish cloud reflected by the two placed side by side, will convince any person that the prism has a decided advantage.

But, besides this inferiority in reflecting power, the silvered diagonal mirror possesses another and far more serious defect; it does not reflect white light unchanged. I have experimented upon this property of the silvered mirrors with the spectroscope, and think that I can perceive indications of a deficiency in the green rays of the spectrum. The simple experiment represented in the following diagram, however, shows the change produced by such a mirror more plainly than any other I have devised.

A is a paraffin lamp; B, a sheet of white card; C, a plane oval mirror silvered on the front or lower surface by Liebig's process; D, a prism made of very pure white optical crown-



glass. The sides of this prism, which are presented to the lamp and card respectively, are ground circular.

The prism and mirror are held, or supported on stands, about three feet from the lamp, and the height of the lamp flame from the card.

Under these circumstances two circles of light may be produced on the card,— one formed by the mirror, the other by the prism.

On comparing the two, it will be found that the circle produced by the prism is white, while that produced by the silvered mirror is strongly tinted with a reddish chocolate colour. This colour is greatly increased if the experiment be varied by causing the reflected light to fall on two silvered mirrors in succession before it forms the circle on the paper.

These are the conditions which obtain in the reflecting telescope, the large speculum representing the first, and the small diagonal mirror the second, reflexion.

The effect of these two reflexions in the reflecting telescope, as usually constructed, is to communicate a reddish tinge to the stars and planets.

Some difficulty has been experienced in attempting to use a telescope mounted in the usual way, for taking photographs; probably the substitution of a prism for the small diagonal

mirror would lessen the difficulty which has been experienced in such experiments.

I have recently had the pleasure of making a very large reflecting prism, $2\frac{1}{2}$ inches in the minor axis, for our late excellent President, Mr. Warren De La Rue; it is now before you. This prism I have submitted to the severest possible tests, and I hope its performance will prove that such prisms are worthy of general adoption.

Note.—Jamin has published an account of some experiments "on the colour communicated to light by successive reflexions from the surfaces of various metals."

RECENT PUBLICATIONS.

Let og Noiagtig Methode &c. Easy and accurate Method for the Determination of the Latitude and Longitude at Mid-day, together with the Error of the Compass, without the help of Logarithms. By J. J. Astrand, Director of the Observatory of Bergen. With three Numerical and two Graphical Tables. Bergen, 1864.

The determinations are effected by means of two circum-meridian altitudes and the meridian altitude of the Sun: thus, if h_1, h_2, are the altitudes before and after the passage of the meridian, and θ_1, θ_2, are the corresponding hour-angles east and west of the meridian (reckoned in time); and if h_0 is the meridian altitude, then the differences $h_0 - h_1$ and $h_0 - h_2$ are proportional to θ_1^2 and θ_2^2, and, consequently, the hour-angle $\frac{1}{2}(\theta_2 - \theta_1)$ at the mean between the two times of observation is given by the formula

$$\tfrac{1}{2}(\theta_2 - \theta_1) = \tfrac{1}{2} \cdot \frac{\sqrt{h_0 - h_2} - \sqrt{h_0 - h_1}}{\sqrt{h_0 - h_2} + \sqrt{h_0 - h_1}} (\theta_2 + \theta_1)$$

where $\theta_2 + \theta_1$ is the interval between the two observations. But the quantity $\frac{1}{2}(\theta_2 - \theta_1)$ is obtained, not by actual calculation, but by means of one of the graphical tables, which, for given values of the differences, $h_0 - h_1$ and $h_0 - h_2$, gives the corresponding value of the coefficient of $\theta_2 + \theta_1$ in the foregoing formula. A correction for change of declination is introduced by one of the numerical tables, and the true local time at the mean of the two times of observation is thus obtained. Thus, in one of the examples given by the author we have

Interval of the observations $44^m\ 36^s$		=	2676^s
$h_0 - h_1$	... ...	=	$39'\ 25''$
$h_0 - h_2$	... ...	=	47 0
Graphic Table I. gives multiplier		=	0,022
$2676^s \times 0{\cdot}022$	... ...	=	$58^s{\cdot}9$
Table III. gives correction		=	+ $5{\cdot}6$
True time	... ...	=	$1^m\ 4^s{\cdot}5$

which, corrected for the equation of time and compared with the Greenwich time, as shown by the chronometer, gives the longitude. The foregoing is a sufficient explanation of the principle of the method.

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